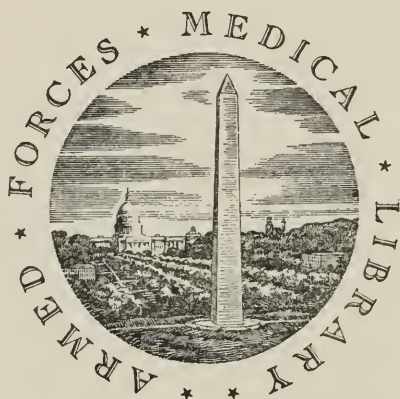




UNITED STATES OF AMERICA



FOUNDED 1836

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WASHINGTON, D.C.





*PATTISON - (G. F. 47)*

**AN ANSWER**

TO

**A PAMPHLET**

ENTITLED

**“Strictures on Mr. Pattison’s reply to cer-  
tain Oral and Written Criticisms,**

**BY W. GIBSON,” M. D.**

Professor of Surgery in the university of Pennsylvania,

---

**BY GRANVILLE SHARP PATTISON, ESQ.**

FELLOW OF THE ROYAL COLLEGE OF SURGEONS, AND MEMBER OF THE MEDICO-  
CHIRURGICAL SOCIETY OF LONDON.

Member of the Wernerian Society of Natural History of Edinburgh. Corresponding Member  
of the Société Philomatique and Société Médicale d’Emulation of Paris. Member of the  
Faculty of Physicians and Surgeons, and late Professor of Anatomy, Physiology, and Surgery,  
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PRESENT PROFESSOR OF SURGERY IN THE UNIVERSITY OF MARYLAND.

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**BALTIMORE:**

**PUBLISHED BY EDWARD J. COALE.**

**R. J. MATCHETT, PRINTER**

**1820.**

1870 and 1871

1870 and 1871

THE UNIVERSITY OF CHICAGO  
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*To Dr. John C. Warren Harvard University  
with the compliments of the Author*

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## AN ANSWER, &c. &c.

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“Yet all this availeth me nothing, so long as I see Mordecai the Jew sitting at the King’s Gate. - - - - - ESTHER, Chap. v. Ver. XIII.

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In the characters sketched in the holy volume, there is an expression of truth and justness which most satisfactorily demonstrates, that it is the composition of that omniscient Being who knoweth all things, and to whom the secrets of all men’s hearts are open. Whether it exposes the nobler failings of humanity, or, as in the passage from which our motto is taken, lays open to us the inward workings of jealousy, envy, malice, and the meaner feelings of our nature, still there is the same truth in the description, the same vigour in the conception, and the same reality in the colouring.

We are told that Haman, the son of Hammedatha, the Agagite, was honoured by the king; who set his seat above all the princes that were near unto him; and that all the king’s servants bowed to him, with the exception of Mordecai the Jew. We are further assured, that his riches were immense and his children many, and that, although he had *neither talents nor merit* to entitle him to such gifts, there was given to him all that the eye could lust after, and all that the heart could desire; yet, because one man lived who would not humble himself before him, in the bitterness of his spirit he exclaimed, “all this availeth me nothing, so long as I see Mordecai the Jew sitting at the king’s gate.”

The lesson taught in this passage is easily read; it teaches us, that although an undeserving character may, from acci-

dental causes, obtain unmerited distinctions, that still these honours avail him nothing, for a sense of his own unworthiness torments him, and makes him look with envy and jealousy on his cotemporaries.

It is not my intention in answering Dr. Gibson's pamphlet, entitled "Strictures &c. &c." to enter into a full, and elaborate, defence of my opinions, as they relate to the operation of Lithotomy.\* Although he has endeavoured to give to his essay a learned dress, its masquerade garb is so ill made, and so inartificially arranged, that every member of the profession, can at once discover that there is nothing of science in its pages;—that it breathes only the vindictive spirit which dictated the disgraceful letters of Aristides;—the same weakness which gave birth to the silly criticism signed W. Had the question therefore, rested only with the profession, I should never have taken the trouble to answer Dr. Gibson's Strictures, but, as these have been most widely, and sedulously circulated for the purpose of attacking my reputation, and, through me, of injuring the university with which I have the honour to be connected, I feel myself bound to expose the falsehood of the charges which have been brought against me. It is very true that the unhandsome, and unprofessional conduct, of the person advancing these, remove in a great measure all credence and force from his statements;—it is evident that they are made, not from a love of truth and science, but, from a malignant and unworthy feeling of jealousy. The author has not heretofore with openness and boldness, attempted to injure me; but by the constant whispering of malicious state-

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\* I am preparing for the press a Volume on the subject of Lithotomy, in which I shall enter into a full detail of the surgical question.

ments, which he himself knew to be without foundation, and by newspaper anonymous scribbling, he has, ever since my settlement in this country, endeavoured to rob me of that which, to every man of sensibility, is the dearest and most cherished of his possessions;—character. It is only now, when dragged from his concealment, and forced before the public, that he has had the manliness to affix his name to any of his scurrilous attacks. I feel too much confidence, in the rectitude of my moral, and the justness of my professional principles, to dread the puny efforts made to hurt me by my antagonist, but I remember the observation of my lord Bolingbroke, “that if calumny is laid on boldly, even by the most worthless minion of society, there is a risque that some of it will adhere,” and therefore, although I feel the most heartfelt contempt for the author of the strictures, I refute them.

The accusations adduced by Dr. Gibson may all be classed under the following heads:

- 1st. That I have been the aggressor.
- 2nd. That I have been guilty of a literary piracy.
- 3d. That I have made incorrect statements.
- 4th. That I have no professional reputation.

In my answer I shall, for the sake of perspicuity, take up the consideration of each of these divisions, separately, and I trust I shall not only be able clearly to prove that these charges are entirely unfounded as they relate to myself, but, on the contrary, that my accuser has himself been guilty of the very crimes he has had the hardihood to impute to me.

In following the arrangement I have adopted, I am in the first place led to the refutation of the charge, that I have

been the aggressor. After reading Dr. Gibson's pamphlet with great attention, I have been unable to discover one argument brought forward by its author, which tends in the slightest degree, to substantiate this accusation, with which he commences. He "hopes, by incontrovertible evidence, to prove every thing he asserts against me," yet neither direct nor circumstantial proof is adduced by him in support of this very heavy charge. The only facts, indeed, which have the most distant bearing upon it, are: 1st. The pitiful complaint made by him, that "I attended his lecture without invitation, and that I posted on the walls of *his* university, an address to *his* pupils, requesting them, at a stated period to listen to a refutation of what he had advanced." 2nd. That I had puffed myself. The first complaint brought forward by the author of the strictures is sufficient to shew that these were written, not for the profession, but for the general public. At the time he wrote it, he must have felt conscious that his brethren, aware, that the medical classes in the university of Pennsylvania are open for the admission of every regular bred physician, would treat it with contempt. Yet in despite of this knowledge he published it, in the vain hope, that the public would consider me guilty of ungentlemanlike conduct in attending his lecture and addressing his pupils. The facts are as follow: for a week or ten days before this lecture was delivered, a report was most sedulously circulated amongst the students, that the professor of surgery was to deliver on a certain day a discourse, which was not only to over-turn my practical doctrines as relating to Lithotomy, but, which was also to prove that I had no pretensions to professional eminence. It was certainly very natural that I should feel anxious to learn the ar-

guments by which the professor proposed to establish his proof, and that, having ascertained their extent, I should be desirous to refute them. I had received at the commencement of the session from Dr. Gibson, a general invitation to attend his lectures. The one alluded to, was delivered for the purpose of over-turning opinions advanced by me, in an open, public, and professional manner. I conceived that a love of truth could alone have induced the professor to question the correctness of my doctrines, and judging of him, as I would judge of a man of honour, I thought that, in attending his lecture, I conferred on him a favour; that, truth being his object, he would be desirous to allow me an opportunity of defending my opinions. Under these impressions I attended the class, in company with Dr. Eberle. His lecture was a very weak and ungenerous attack upon my reputation. It is true that my name was not mentioned, but it was quite evident to all the students that the single object of it was to convince them that I had been guilty of most disingenuous conduct in relation to Mr. Colles, and, of great ignorance as to the principles and practice of my profession. Although the arguments advanced by the professor were so puerile and contradictory, as to carry along with them their own refutation, still, in justice to my public character, I felt it my duty to answer to *my* students, who formed a part of *his* class, the charges which had been made insidiously against me. In the course of the same day I had therefore posted up in the gate of the university, a M. S. bill, which simply stated, that I would the following evening deliver a lecture on the anatomy of the prostate fascia, to which the medical students, and profession generally were invited.

As the doctor appears offended at my having attended his



lecture without a more particular invitation, I, without hesitation, apologise to him for doing so. This lecture had been so long and so well published, that I had my professional curiosity excited, and really could not exercise that self-denial which he showed when under a similar excitement. If he thinks I acted unmannerly in coming into a *public class room* without a card of invitation, he must allow that I took care to avoid placing him under the painful necessity of either being guilty of a similar unpoliteness, or of having a laudable curiosity remain ungratified. I sent him a very civil card, inviting him to attend my lecture, delivered with the view of refuting the opinions he had advanced. He, however, did not think fit to avail himself of it.

My discourse was not delivered as an attack on Dr. Gibson, but as a defence of my own opinions: I did not speak of him disrespectfully, but merely endeavoured to convince my auditors, that nothing he had said, could have the effect of taking from me the claims I had made, or of disproving the justness of my practical doctrines. Had I felt desirous to crush my invidious enemy, the field was open for me; I might have exposed his malevolence, by laying open and enlarging on the object which had dictated his lecture,—I might have demonstrated the weakness of his mind, and his want of originality, by reading from authors whole pages he had got by rote, but he was a *professor*, and I felt pity for him.

Having thus disposed of his first complaint, and proved that it is groundless, I am next led to the refutation of his second;—that I puffed myself, and wished to introduce into the American newspapers a system of puffing.

I am aware that it is necessary to be a little hypothetical, in order to construe this complaint, into an aggression

against Dr. Gibson. But, allowing latitude to my argument, I may suppose, that the offended individual might conceive that it would be an offence against him in two ways; first, that it might have the effect of pushing me forward in practice, to his detriment, and secondly, that the indelicacy of it hurt his feelings.

Every person who has read the "Strictures" where this puff is so loudly and repeatedly complained of, must have been led to suppose, that it was one of a very offensive nature. The best method of contradicting this is to, publish it.

*From the Norfolk Beacon.*

## THE AMERICAN MEDICAL RECORDER,

*Published in Philadelphia, by James Webster.*

Periodical Journals, when properly conducted, are, of all publications, the most interesting to those who live remote from the sources of science and information.

Through such publications the streams of knowledge and improvement flow in innumerable channels, and may be brought to every door, without either much trouble or expense.

I am led to these reflections by having just read the last number of the above named interesting medical journal, which, judging from this specimen, I regard as highly creditable to the editor, and very worthy of an extensive patronage. This number contains several papers, possessing a very high degree of interest. The initial article, written by Mr. G. S. Pattison, surgeon, is without doubt the most important paper on the subject of *lithotomy*, that has appeared in any country, for a considerable time past. There is



another paper, by a Dr. McClellan entitled to considerable praise. It relates to the "surgical anatomy of the arteries." It brings forward some new ideas relative to the anatomy of the pleura, and discovers generally an acute and discriminative mind. There are several well written reviews in the number, and the selections and general management of the work appear judicious.

I have made these remarks, influenced by no other motives than the desire of directing the attention of physicians to the work, which I verily believe, to be fully entitled to their patronage.

MEDICUS.

It is obvious to every person, who reads this letter, that its single object and intention, is to puff the Medical Recorder, not Mr. Pattison. I am introduced only incidentally with my friend Dr. McClellan, and the editor, who are equally puffed with myself. I may state, that at the time this letter was published I had no interest in, nor the most distant idea of ever being connected with, the American Medical Recorder.

It will be unnecessary for me, I conceive, to employ any arguments to show that there was nothing very offensive in the communication published in the Norfolk Beacon; but, allowing that it had been composed in a very different spirit, and had blazoned forth in the most empirical style my merits, Dr. Gibson was not entitled to urge its appearance against me, as I shall, by the publication of the following letter, prove that he had the most positive, and satisfactory evidence, that I knew nothing of it until after its appearance in the newspaper, and that I had then, in the strongest terms, expressed my disapprobation of all such publications.

Philadelphia, Sept. 15, 1820.

“DEAR SIR—I observe, that Dr. Gibson in his *Strictures*, &c. still persists, very disingenuously, to accuse you of having written the Norfolk puff. He says, “I can only say I verily believed him to be the author, and I have as yet *no good reason* to change my opinion on the subject.” More than six months ago, I assured Dr. Gibson, *upon my honour*, that you had nothing to do, either *directly* or *indirectly*, with the publication of that *offensive* article, and declared to him, that I could speak positively upon the subject, as I had a perfect knowledge of the source from which the puff emanated. I subsequently frequently repeated to him these assurances, under pledges, such as a man of honour deems sufficient to give, or to receive upon any occasion. The last time I thus solemnly assured the Doctor of your innocence, of the unhandsome charge he has been constantly urging against you, was in the presence of Dr. Chapman. In addition to all this, Mr. Webster offered Dr. Gibson to make oath, before a magistrate, that you had nothing to do, either directly or indirectly, with the puff in question. This surely was enough to restrain, any liberal minded man from repeating this gross and unjust accusation; but, however insulting it be to the feelings of an honourable man, to have his most solemn pledges thus lightly estimated, I refrain from making any further remarks on the flagrant injustice which is done you, and the insult offered me, in the present instance.

As to Dr. Gibson’s assertion, that your conduct has been *empirical in the extreme*, and that the profession in general in Philadelphia, can bear testimony to this, I can only say,

that, from what I know of the sentiments of my professional brethren in this city, I have every reason to believe, so far from considering you as having acted empirically, since you have come amongst us, they approve of your conduct, as highly honourable, and strictly professional.

I am your friend, &c.

JOHN EBERLE."

*To Granville Sharp Pattison, Esq.*

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That puffs concerning the operations of American surgeons are occasionally published in the newspapers, is a fact well known.

If the Baltimore papers, for a few years back, be looked over, it will be *observed* that some of the operations, of the same Dr. Gibson, who felt so terrified that I should introduce a "*puffing system*" into this country, are extolled in terms of the most absurd hyperbole: and further, if he only takes the trouble to look into Mr. M'Corkle's paper of May 31, 1820, and the other city newspapers of the same date, he will find that there are published on his friend, *who stands as the first surgeon in the country*, and who is not to be benefited by any praises of mine, the following remarks, for the performance of one of the most simple and trifling operations in surgery.

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*From the Freeman's Journal of May 31, 1820.*

"RESTORATION OF EYE-SIGHT.—We are gratified in being enabled to communicate to the public a remarkable cure which has been effected upon the eyes of Mr. Robert Lauier, a gentleman well known in this vicinity.

Mr. L. for a number of years had laboured under the almost total deprivation of the sight of his right eye, the left being entirely destroyed for about nineteen years past. The most skilful surgeons had been consulted, but no relief could be administered till within a few weeks past, when he went to Philadelphia, and applied to Dr. P. S. Physick, who, in four seconds, removed the cataract, and restored the defective eye almost to its original state of perfection; and this too, without the least pain or inconvenience. Mr. L. returned home on Friday last, and has requested us to make public the cure that has been effected by Dr. P. not only as an act of justice towards that eminent physieian, but for the benefit of such of his fellow-citizens as may labour under a distressing visitation of a loss of eye-sight."

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It is not my wish that it should be thought Drs. Physick or Gibson were the authors of the puffs, which have repeatedly appeared concerning their operations; I am satisfied that the former gentleman, would not give his consent to the publication of those, which related to him, and had the latter individual not affirmed, in his "*Strictures*" that such could not appear without the authority of the person puffed; "and, that there are ways in which such an affair can be managed, so as to remove the odium of it from his own shoulders;" I should never for a moment, have entertained the idea, that *even he*, would have assisted in composing those which have appeared regarding himself, in the Baltimore papers.

I feel satisfied from the few observations I have made, that my readers give me an unanimous verdict, "*not guilty*," on the first charge, and come now, with equal confidence, to re-

quire of them a verdict of "Guilty" against the author of the "*Strictures*."

For the purpose of obtaining this verdict, I shall not go beyond the present controversy. I might, were I desirous, prove that from the hour I began to lecture, Dr. Gibson, by the circulation of childish and malicious whispers, has done every thing, in his power, to injure my character. I have however no occasion to bring forward these, in support of my case. From the history of this controversy, I can obtain much more evidence than is sufficient to convict him.

I do not suppose that Dr. Gibson himself will have the hardihood to deny, that his lecture was a concealed, and, on his part, a most indelicate attack upon my reputation; granting, for a moment, that my opinions were indefensible, and that I had been guilty of a *literary piracy*, from the peculiar relation which existed betwixt us, had he possessed any delicacy, *he*, certainly of all men, was the last who should have exposed me. But one attack was not sufficient, writhing under the disappointment that I had triumphantly refuted the arguments, which he hoped would have crushed me, he departed so far from all those principles, which are acknowledged as guides for regulating the conduct of gentlemen and men of science, as to publish in Mr. Poulson's newspaper the disgraceful letters signed "*Aristides*." I am astonished that Dr. Gibson has dared to come before the public, and acknowledge that he was their author;—and what is his apology? "*a sort of quack bill or circumforaneous advertisement*," he, observes "*emanating apparently from a Norfolk newspaper, was published successively in most of the Philadelphia newspapers, &c.*" This sort of quack bill,—this circumforaneous adver-



tisement, which he has the impudence to plead, as the single cause, which justified the publication of his anonymous attacks, was the *communication* already published;—a *communication* which contained nothing, which could have the effect of injuring his character, and one, which he had the most satisfactory evidence, was neither composed nor published by me. He tells us, that he left his name at the office of the newspaper, making here a virtue of a necessity, for where is the printer who without a name, would have made himself responsible for such an assassin-like attempt on the reputation of any man?

I can well believe that, had Dr. Gibson been aware, I was in possession of all the facts which I am about to state to my reader, he would never have referred in his *Strictures* to Aristides's letters. I may preface these by observing, that I am authorised by the gentleman whose name is mentioned, to make the following statement.

Having published the first of the letters signed Aristides, so well pleased was he, with his production, that, on the morning on which it appeared, he sallied forth to ascertain what the public thought of it. Early in the day he called on Dr. Eberle, and, with apparent satisfaction, asked him if he had read "*the cutting up I had got in the newspapers.*" Being answered in the affirmative, he enquired the Doctor's opinion of Aristides's letter. Dr. Eberle, suspecting that he was its author, thought he would hold out a *bait* for him, and in reply, said, "*It was very clever.*" The *bait* took, and the professor at once declared, "*I wrote it.*" Dr. Eberle, having gained his object, assured Dr. Gibson that he conceived he was acting a very unhandsome part to Mr. Pattison. The answer made was, I had no business to publish the puff about myself in

the Norfolk Beacon. The most solemn assurance was given him, that I had no knowledge of that communication, that some of Mr. Webster's friends had inserted it, for the purpose of establishing the character of the journal, and that, if required, Mr. Webster would give his oath, that I never heard of the letter before it was printed, and that I had then expressed, in the strongest terms, my disapprobation of all such publications. The apology he had pleaded in excuse, being removed, he was obliged to shift his ground, and contradict his former statement, by declaring he had not written the letter himself, but that it had been composed by one of his students, and that he, the *Professor*, had merely acted as his pupil's *amanuensis* and *porter*, having copied it, and carried it to the printing office!!!

When I saw the first of Aristides's letters, in the newspaper, I certainly did not suspect that it was the production of a PROFESSOR. I treated it with contempt. My brother, however, felt it otherwise, and calling, without my knowledge, on Dr. Eberle that day, (Friday) observed to him, that he thought some of my professional friends should answer it, which that gentleman readily agreed to do. When my brother on Sunday told me what he had done I blamed him very much, and immediately left home for the purpose of seeing Dr. Eberle, and requesting him to stop the publication of the defence, he had sent to the newspaper. Not finding him on my first visit, I wrote a note, requesting him not to take any notice of the pitiful letter, written by Aristides, but, fearful that this might not induce him to comply with my wishes, I called again late at night, and begged him to withdraw from Mr. Poulson, the letter he had written in my vindication, as I had no wish that my name



should be brought before the public in a newspaper. He assured me he would do so, but observed, that it would be unnecessary to call at the office before the morning, as he had been informed by the gentleman who had the charge of it, that the letter could not appear before Tuesday morning's paper. From some alteration in the arrangement of the Monday's paper, the letter, which it was said could not appear until Tuesday, was, contrary to our expectations and wishes, printed.

A few days after Dr. Eberle's letter signed Z. had appeared\* Dr. Gibson called on him with a second communication which he had prepared for publication. He thought he had now a good excuse to vent his spleen; for who could be the author of the letter which vindicated my claims, but myself? This second communication he read to Dr. Eberle; it contained a very violent, and abusive attack on my character. That gentlemen begged it might not be published, and assured him again and again, in the most solemn manner, that I was not the author of the reply to Aristides. On Dr. Gibson's continuing to assert that he was convinced that I was its author, Dr. Eberle, with the most honourable candour, told him that he had himself written the letter signed Z. and then related to him all that had occurred, to convince him that any defence of that kind was contrary to my inclinations. Having received such a satisfactory statement, what did the author of the "Strictures" do? Did he feel ashamed of his conduct, and come and ask pardon of the

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\* Dr. Eberle's letter contained only a few temperate remarks on the impropriety of making any observations in a newspaper on a scientific subject; and observed that, as my paper was published in the Recorder, only for the profession, this journal was certainly the proper organ for the publication of criticisms upon it, &c. &c.

man, whose character he had mistaken, and whose reputation he had attempted to destroy? No, he goes home, vexed and disappointed, because my character was not what he would have wished it to have been; he prunes his letter of some of the more odious personalities and abuse which it contained, and sent it to the press, that it might represent me, as an *ignorant pretending quack*, to the commercial public of Philadelphia. With a knowledge of all these facts, Dr. Gibson asserts, in the first paragraph of his "*strictures*," that he shall show, "by incontrovertible evidence, that his conduct, from beginning to end, has been UPRIGHT and HONOURABLE!!!"

Were I to adduce, in support of my assertion, that Dr. Gibson was the aggressor, all the proof I am possessed of, I should fill a volume. But I am persuaded that the few facts I have stated, are sufficient to make good my case, and to satisfy every reader, that unprovoked envy and jealousy, have characterized the behaviour of my antagonist, from the beginning of the controversy. I shall therefore now conclude the first division of my "answer" by stating a few facts, which, I trust, will prove, that, although aware of all the injury which Dr. Gibson has attempted against me, I have only felt pity for his malevolence and weakness, and that, so far from wishing to expose him, I have only done so, as far as was necessary for my own justification.

In the 10th number of the Medical Recorder, the author of the "*strictures*," published a case of "tying the iliac artery, with observations." Never perhaps, was a paper more defective, and more open to just criticism, than the one alluded to. Yet so far was I, from wishing to interfere with this man, who had constantly since my settlement in the country, been

endeavouring to injure my reputation, that I did not write a criticism upon it myself, and further, employed all my influence with the editor of the Recorder, to prevent him from publishing, two very severe criticisms upon it which were offered by very intelligent surgeons.

If my "*reply to certain oral and written criticisms,*" &c. be perused it will be found that it contains only an independent defence of my opinions. I no doubt speak in terms of strong and just reprehension of the conduct of Aristides, but although it would have been perfectly fair in me to have mentioned who this anonymous traducer was, on this I was silent.

I am informed that Dr. Gibson has been very much irritated at me, for illustrating the weakness of the criticism signed W. by saying that it brought to my mind *an old story* of a professor, who, finding it no easy matter to lecture, had apologized to his class for not giving them a valedictory lecture, saying, "he had intended to have written them a very *fine* lecture, but that really he was so morally and physically exhausted, that he found it impossible for him to compose one." The professor has however himself to blame for allowing the joke to become public, for I can most solemnly declare, I never, until after the publication of my reply, thought that he would have been so well pleased with this valedictory, as to repeat it with considerable self satisfaction to his friends. Had he only kept his own secret, the public would never have come to the knowledge, that my illustration meant more than appeared.

I shall preface my defence on the 2d charge, "that I have been guilty of a literary piracy" by a very abridged history of my first essay.

I was educated in the belief, that it was proper, in operating for lithotomy, to make large wounds into the bladder, I thought the operation correct, and for the first years I lectured, I taught it as such to my students. A fatal case occurred, where the operation was performed with great ease and rapidity by a large wound, and where nothing happened to explain the death. As I had lost a patient from urinal effusion, a short time before, and, as there was a very marked resemblance in the symptoms preceding dissolution, in the two cases, I suspected, that in the lithotomy patient, urine might have been infiltrated, and might have operated as the cause of death. Under this impression, I conducted the dissection, not as it is usually performed, but with the view of ascertaining, whether there had or had not been urinal infiltration. I discovered this had really occurred, and that my patient had died from it there could be no doubt.

I was satisfied, in my own mind, that the urinal infiltration in the case mentioned above, had produced the fatal event, but I had at that time no idea that the largeness of the incision had any effect in producing the infiltration. Dissecting, afterwards, the bodies of those who had died, in the practice of my friends, from the operation, I invariably found pus betwixt the *bas fond* of the bladder and the rectum. This I was aware was produced from the effusion of urine, but as I had no suspicion that this would occur more readily after a large, than a small wound, I continued still to recommend a large one. A considerable time afterwards, Scarpa's Memoir on Lithotomy was brought me. That great surgeon asserted that urinal infiltration was the common cause of death occurring after the operation for stone; my experience confirmed this, but Scarpa's went further than I had done, he



affirmed that *his experience* had proved to him, that this was more apt to follow a large, than a small wound. As he, however, gave no anatomical reason, why it should be so, I was unwilling to credit the statement. It occurred to me at the moment that if I could find a fascia, acting as a septum betwixt the cavity of the pelvis, and the perinæum I should have all my doubts resolved, and could then, on philosophical principles, recommend the practice of operating, advocated by Searpa. I did find such a fascia, and *I thought* I had made an *anatomical* discovery. Fearing that I had heretofore, in my public teaching, misled my pupils, I gave up all my favourite predilections, and taught, what I considered a more correct doctrine. Sometime after my arrival in Philadelphia, I read Mr. Colles, and discovered, that, that anatomist had seen and described the prostate fascia. This I made public, and, in the 9th number of the Medical Recorder, published an essay in which I brought forward my opinions, for the examination and consideration of my brethren. This, "*is the very head and front of my offending.*"

Having thus in a very few words stated the general facts of the case; I am led to insist more fully on the fact, that I have not been guilty of a literary piracy.

Dr. Gibson, in his anonymous characters of Aristides and W., has, in the face of the most *direct evidence* to the contrary, asserted, that I have claimed the discovery of the prostate fascia, and, even after my reply, in which I adduced direct proof that I had not done so, he continues in his "*Strictures*" to make the same incorrect statements, and attempts by quoting garbled extracts from my first essay, to prove them. And although I, in my "*reply*," gave a perfect explanation of these expressions he has been so disingenuous, as never to advert to

it. That the general reader may clearly understand this part of the controversy, I shall, I trust, be excused for repeating some of the facts which I published in my reply."

Conceiving that the clearest way of conveying to my readers mind, the progress of my experience, I delivered it in the form of a diary of my thoughts, and as the prostate fascia was *really and truly*, to me a discovery, *until* I read Colles in Philadelphia, in speaking of my experience, at the date when I first dissected the fascia, I spoke of it as a "new fascia, a fascia which I called the prostate fascia." In continuing my account, however, I with equal clearness state that I was not the discoverer of the fascia, I observe, in the plainest language, that *after* reading Mr. Colles in Philadelphia, I was satisfied that Mr. C. had seen it before me, and finally concluded my essay, by giving up all pretensions to the anatomical discovery, stating, "THE ONLY CLAIM I SHALL MAKE, AND IN THIS I AM CONFIDENT I SHALL BE SUPPORTED, IS, THAT, UNTIL THE PRESENT, NO RATIONAL EXPLANATION HAS BEEN GIVEN, OF THE MANNER IN WHICH URINE IS EFFUSED, AND CONSEQUENTLY NO OPERATION HAS BEEN PHILOSOPHICALLY PROPOSED TO PREVENT IT." One would suppose that the amount of what I claimed was sufficiently limited and defined in this the concluding paragraph of my *first* essay, but, although I reprinted it in *Italicks*, in my reply, still, Dr. Gibson, in his "strictures," continues to abuse me, for attempting to take the credit of the discovery of the prostate fascia to myself. That he himself is satisfied that this claim has never been made by me, I am persuaded, but, aware, that he could not take from me that which I really claimed, he has hoped to deceive the *general public* and make them believe, that I have been guilty of a literary piracy.

In attempting to do so, he has unluckily quoted a passage from my former essay, which, if he had considered for a moment, was of itself sufficient to prove the fact of my not claiming the prostate fascia as a discovery. "I presume" observes Dr. Gibson, "that he cannot deny that he (Mr. Pattison) stated, I still continued to believe it was so (i. e. a discovery) until I read Mr. Colles in Philadelphia," what is the clear inference to be drawn from this quotation of mine, surely, it is that *until* I read Mr. Colles in Philadelphia, I thought I had made an anatomical discovery, but, having done so, I no longer continued in that belief. The *ingenious* professor, however, draws from this passage a very different conclusion. He thinks I meant not to allow that Mr. C. knew any thing of the prostate fascia, but, merely, to insinuate, that I had never read the work before. He triumphantly states to his reader, that he has discovered the secret, that I had read Colles shortly after it was published. Now this secret, which he wishes it to be supposed, I had no desire to be known I did not reveal to a single confidant, but to a class of 300 hundred students, and to my professional friends generally. I never thought, that making the acknowledgment I had read the book before, was sufficient to prove, that I must necessarily have been aware of all the facts which it contained. I think I can easily prove, that a person may read a book and still not bear in mind all its contents. I shall be excused I hope in offering two respectable illustrations in support of this position.

1st. Every person who has been in the habit of attending Dr. Physick's lectures, must know, that annually he has been in the habit, of recommending the employment of *animal ligatures*, and of taking very considerable credit to himself as



their discoverer. Dr. Gibson, last session, extolled these ligatures, and abused Mr. Astley Cooper of London for using them, without sounding the praises of his colleague. We can hardly suppose that either of these individuals, are so ill informed on the works of their profession, as not to have read "YOUNG'S MEDICAL LITERATURE" where we have the practice of animal ligatures recommended, on the same principles as those, which regulated Dr. Physick, viz. that they might be absorbed again. Dr. Physick has taken great honour to himself, for being the discoverer of an instrument for tying arteries in deep situations. The author of the strictures, speaks of it in terms of high commendation, in his present pamphlet, calling it "*Dr. Physick's forceps and needle.*" If "*Heister's Surgery,*" which is one of the oldest text books, be opened, there will be found in it, not only a description, but an *engraving* of an instrument of which Dr. Physick's is a mere modification. It is true, that the needle figured by Heister is not recommended for tying *deep seated arteries*, but for sewing *deep seated wounds*!!

These illustrations are not brought forward as *offensive*, but as defensive arguments; I do not wish to prove that Dr. Physick was desirous to purloin these discoveries from those to whom they belonged. My only wish is to show, that it is possible for an honest and intelligent man, to read a book and yet not be fully aware of all its contents. When I read Mr. Colles in Philadelphia, and discovered that he had described the prostate fascia, previously to me, I immediately proclaimed it. This conduct was, certainly candid and honourable towards Mr. Colles, but, although the claims of HEISTER and YOUNG, have been pointed out to Dr. Physick, I have not yet learnt, that he has come forward and given up with equal candour *his* pretensions.

As Dr. Gibson has never brought forward a new thought nor an original idea, on the subjects of his profession, I have no power to test his discoveries.

From the quotations brought forward, in the "*Strictures*," it would, at first sight, appear that the prostate fascia was familiar to all the best anatomists, and that it indicates ignorance in me, to have ever supposed I had made a discovery, when the same parts had been described by Santorini, Winslow, Fyfe, Munro, &c. &c.\* If the description given of the part by these anatomists be referred to, it will be observed, that what Colles describes as a fascia, they describe as ligaments. Santorini names it the ligamentum prostatæ novum, Winslow speaks of it as a tendinous ligament, Munro, likewise, employs the term ligament, when he mentions it. Fyfe comes nearer the truth; he calls it, a tendinous expansion, but, in his description, he merely describes it as being inserted into the arch of the pubis. Sabatier divides the fascia into *distinct* ligaments. In all the authors who have mentioned this stricture, with the exception of Mr. Colles, the prostate fascia is not described as a septum which separates the

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\* As it is pleasing for us to fulfil our duty to every man, we cannot allow the present opportunity to pass without acknowledging that we have never read any essay on lithotomy, if we except the article on this subject in the *Dictionnaire des Sciences Medicales*, where an equal degree of learning, or the same number of illustrious names are given, as in these strictures.

The following is taken from a single paragraph of the above named pamphlet.

Cheselden, Douglass, Sharp, Daunt, Dease, Sir James Earle, John Bell, Dr. Thompson, Allan Burns, Charles Bell, Allan, Dionis, Le Dran, Dechamps, Sabatier, Boyer, Bertrandi, Desault, Hunter, Cline, Carlisle, Lynn, Abernethy, Home, S. Cooper, Lawrence, Black, Blizzard, Pearson, Benjamin Gibson, Hey, Brown, Newbigging, Russell, Inglis, George Bell, Colles, Barlow, Foster, Chevalier, Nannoni, Flajani, Pelletan, Percy, Richerand, Patrizi, Lallemand, Roux, Cloquet, Las Frank, Serrurier, Merat, Guillé Le Roux, Beclard, Delpeck,——— but this is really fatiguing, and I must relieve myself by the exclamation of my very learned friend Domine Sampson, "PRODIGIOUS !!!!!!"

cavity of the pelvis from the perinæum, but as forming the ligaments of the bladder. The anatomists who described it, considered it as forming ligaments, they dissected it, accordingly, and, as the term ligament naturally conveys the idea of an isolated band, they divided it into distinct portions. From the ideas they had of its structure and use, they deceived themselves as to its true nature and importance, and, from the term they employed in its description, I was misled in the first instance myself, and supposed I had made a discovery, when in fact I had only saw correctly, what they had seen incorrectly.

That I was not solitary will appear when I state, that all the anatomists to whom I demonstrated this structure, previously to my reading Colles in Philadelphia, when I myself ascertained that I was not the discoverer, considered me as such. Dr. Gibson insinuates that both Dr. Physick and himself were perfectly familiar with the prostate fascia before my arrival in the country. I deny the insinuation. I assert, that neither the one, nor the other, had an idea of such a structure, until their attention was directed to it by my demonstrations and essay; and I do not believe that even Dr. Gibson will have the boldness to come forward and declare, that he really was aware that such a structure existed. There are two facts which will at once disprove such an assertion, supposing it to be made by either of the above named professors. Last winter, (and *never* before last winter, was a demonstration of the prostate fascia given in America,) it was demonstrated twice, first by Dr. Physick, and afterwards by Dr. Gibson. That the former considered it my discovery, up to the hour when he described it to his class, is evident, although I had done all in my power to convince him

that I had no claim to the anatomical discovery, from the manner in which this is noticed by the author of the "*Strictures*." "The fact is, Dr. Physick attached so little importance to the fascia, that he merely showed the part, and read a passage from Mr. Pattison's paper, without a comment."

Dr. Gibson is so very weak, and inconsistent, in insinuating his and Dr. Physick's knowledge of the fascia, that I cannot deny my reader the amusement he will derive from the absurdity of his argument.

He begins it by observing, during the time he was pushing his canvass, for the chair of Surgery in the university of Pennsylvania, he heard that Mr. Pattison had made a brilliant discovery!! His whole mind, he tells us, was excited by the most lively interest to ascertain its nature. I can believe, that a man ardent in the pursuit of professional information may have felt, as he describes he did feel, but, really, I cannot believe, that had he felt so, he would patiently have continued for five months, until the publication of the 9th number of the Recorder which contained my essay, without satisfying his "lively interest on the subject." He had an opportunity of ascertaining the fact, by attending any of the demonstrations which I was in the habit of occasionally giving, or of dissecting the parts on a subject himself; but, no! the *inquisitive professor*, continued to keep his mind on the stretch, until my essay began to excite a very considerable interest amongst the students and profession. This had the effect of putting a complete stop to his pretences, but he was not then aware, although he asserts that he was so in his "*Strictures*," "that I had only given an imperfect and awkward description of a structure about which most of the best anatomists had said more or less," on the contrary, as Doctors



Eberle, and McClellan, and many others can testify, he went about sneering, and denying that the prostate fascia existed. When he was told by these gentleman that there was a fascia, he boldly asserted, that, they were deceived, observing, that any man, who could handle a scalpel, could easily make one, so as to deceive lookers on. At this period of the controversy, subjects were dissected in the rooms of the university, but, not with the view of showing the pupils that I had claimed as a discovery what had not belonged to me, but, for the purpose of convincing them, that, the part described, had no existence except in my imagination.

As the author of the "*Strictures*" proceeds, he continues to be equally absurd and contradictory. A fortnight before he delivered his lecture on lithotomy, he tells us, that he went into Messrs. Collins and Croft's bookstore, and purchased Colles's Surgical Anatomy. He discovers in it "*an unusually full and well written account of those very parts, which Mr. Pattison had claimed as a discovery of his own.*" To me, it appears very strange that Mr. Colles's description of them should have received so much of his attention. He has informed us, just before, that Dr. Physick and *himself*, in a word all the great anatomists, were perfectly acquainted with the part I had claimed as a discovery. That his statement is incorrect, will be proved, of necessity, by his own account of the business, for, in spite of what he had before said of his own and his colleague's knowledge of this structure, he proceeds to inform us that they both examined Mr. C's. unusually clear description, with dissections made by Dr. Physick's dissector. When a man of talent supports assertions, which he is aware are false, we may be amused by the ingenuity and consistency which he throws around his argu-

ment, but, when a weak minded individual attempts to support that which is not tenable, we become at once disgusted by his feeble and contradictory efforts. I shall, therefore, no longer fatigue my reader's attention by dwelling on this part of the "*strictures*" but shall merely in conclusion ask the question. Is it probable, had either the one or other of these individuals known the prostate fascia, that it would have been necessary, before they could ascertain whether the one described by Mr. Colles was the same mentioned by me, for them to hold a consultation, and make dissections?

Having thus established the fact, that, although I was not the discoverer of the prostate fascia myself, still it was a structure of which they had no idea, until after my arrival in America. I come to conclude, this second division of my answer, by making a very few observations on the nature of my practical deductions, and on the arguments which are brought against them, by the author of the "*strictures*."

Scarpa, one of the greatest surgeons of the present age, has been, from experience led to assert, that the infiltration of urine betwixt the *bas fond* of the bladder and the rectum, is one of the principal dangers to be dreaded, after the operation of lithotomy;—all intelligent surgeons agree, that patients often die after this operation, without there being found those marks of inflammation in the abdominal viscera, necessary to account for that event;—In all the examinations I have had an opportunity of making, after death, from lithotomy, I have dissected the bodies, not as is usually done, but, with the view of discovering urinal effusion. I have invariably discovered that it was present, and that it had been the cause of death, there is every reason to believe. I have demonstrated,

from the connections of the prostate fascia, that it forms a perfect septum betwixt the cavity of the pelvis and the perinæum, and that, so long as the base of the gland remains uncut, it is *physically impossible* for one drop of urine to infiltrate betwixt the base of the bladder and the rectum, and consequently if infiltrated urine be a principal cause of death, the operation recommended by me is free from this danger.

Dr. Gibson has taken a great deal of pains to prove, what I have no wish to deny, viz. that many of the best surgeons, of the present day, advocate a practice very different from the one I have recommended. It is only since the publication of my essay, that the fact, that urine may infiltrate after a large wound, but that it is impossible to do so after a small one, where the base of the prostate gland remains undivided, has, from the demonstrations of anatomy, been proved.

Had I not been able to do this, and upon philosophical principles recommend my operation, I should have continued a disciple to the principles in which I was educated. To say, because the majority of surgeons are of a different opinion from me, it is necessary that my principles should be erroneous, is to reason like a child. When Harvey published his work, "*De motu cordis, et sanguinis circulatione*" all his professional brethren, to a man, declared that he was mistaken. Did the event prove that he was so? When John Hunter first recommended the tying of the femoral artery, in cases of popliteal aneurism, were not his views contrary to those of his contemporaries? The honour of a discovery rests in his offering to the world something which is contrary to the general and received opinion. If experience should prove that my practical conclusions are correct, I shall be



entitled to very considerable credit; should it however demonstrate, I am wrong, all that can be said, will be, that I, like many others, have been mistaken.

I have already stated that it is my intention to publish a volume on the subject of Lithotomy. I had not intended to enter on a refutation of that part of the "*strictures*" which may, by the uninformed, be considered as bearing against the justness of my practical conclusions. I am however tempted, to make one or two remarks on the author's argument on this division of his subject, merely, to show that he is here as inconsistent and contradictory, as in the other parts of his pamphlet.

Dr. Gibson takes it for granted, because Dr. Douglass says that Mr. Cheselden cut the prostate gland and shoulder of the bladder, in his last and most successful operation, that he *must* have done so. This assumption is not admissible, for, if the arguments which I adduced in my former essay, had the effect of rendering it probable, that Mr. Cheselden was himself deceived, as to the exact extent of his incision, surely it is not inferring too much, to suppose that Dr. Douglass, who merely described the operation of Mr. Cheselden, from the account which that gentleman gave of it himself, might be incorrect. The only argument which the author of the "*strictures*" brings forward to overturn the reasonings adduced by me in support of my opinions, that Mr. Cheselden was deceived, is, "that I shall find it no easy matter to make other people suppose, that Cheselden, a great anatomist, was mistaken." Will the Doctor allow me for a moment, *merely for the sake of my argument*, to say, that he himself is one of the *first* anatomists of his age. Taking this for granted, I can prove that even a *very great man*

may, in his operations, cut parts he does not intend. Now, for my story, which is not made merely for the sake of argument, but which is a true and well substantiated fact. Dr. Gibson, anxious to make converts, has of late taken every opportunity to endeavour to convince the students, that I am mistaken. He, *very candidly*, mentions in his pamphlet, one operation, performed with this intention, in the Philadelphia alms-house, but, *very delicately*, considers it needless to say any thing of the first one he performed there with the same intention.

Proceeding, with the view of convicting me of error, he cut into the perinæum, until *he thought* he had found the membranous part of the urethra, and then thrust his gorget into the wound, *supposing* he had cut only a portion of the prostate gland. After he had executed the operation, perfectly satisfied with himself, he walked up and down the room, declaring he would stake something to which he seemed to attach great value, perhaps it might be his reputation, if the instrument had divided the base of the prostate. This harangue was, however, soon interrupted, the woe-expressive countenance of the dissectors denoting that all was not right. The professor became pale and agitated, he thought the gorget had not done its duty, and that, unfortunately for his opinion, the base of the gland was cut. But, this fear was groundless. The Doctor had never found the gland, and had, without being aware of it, in spite of *all* his anatomical and chirurgical knowledge, carried his gorget directly into the shoulder of the bladder.

I am aware, that there is no parallel betwixt this inexcusable blunder of Dr Gibson's, and the opinion advanced by me that Cheselden did not, in every case, extend his incision so

far as he intended. Experiment will prove, that the very best anatomists and surgeons, in performing the last of Cheselden's operations, will very often discover, by dissecting the parts afterwards, that the shoulder of the bladder remains undivided; but I readily allow that no man, who has the slightest pretension to anatomical knowledge or Surgical dexterity, will ever be guilty of the blunder committed by the professor of surgery, in the University of Pennsylvania. That it did not injure the dead body, I am willing to admit, but, that it would equally have occurred, had the subject been alive must be evident, and I will assert, that had this been the case, the life of the patient, would, in all probability have fallen a sacrifice to the ignorance of the operator.

Allowing for a moment, that the author of the "*Strictures*" is correct in his assumption, that Cheselden did, in every instance, cut the whole of the prostate and shoulder of the bladder, this does not prove, that such an operation is the best, which can be executed. He admits that, from this *perfect* operation, Cheselden lost 20, out of 213 patients; and, in the very commencement of the same paragraph, informs us, that Rau, the Dutch Lithotomist, did not loose a single patient, although he operated 1540 times. It is a monstrous absurdity to assert, that an operation is perfect, and the best which can be performed, when it is admitted, 1st. that 20 die from it out of 213, and, 2dly, that another has been executed, where there was not a single death in 1540 cases. I would ask Dr. Gibson, as an *honest man*, what operation was left for Rau to perform, but the one I have recommended. That he did not open the shoulder of the bladder, leaving the prostate untouched, the melancholy result of the first of Cheselden's operations, where

this practice was adopted, is sufficient to prove; that he did not cut the shoulder of the bladder and prostate gland, is equally demonstrated, for supposing Cheselden, in his last operation, did so in every instance, the result is very different, 20 patients died from the operation, out of 213. There was only one operation left for Rau, which was to make a small wound in the prostate, leaving its body uncut, and thus render any urinal infiltration *physically impossible*.

I would not pretend to assert, that no other cause but that of effused urine, can operate as the cause of death, after the operation for stone. Dr. Gibson observes, that he has seen in Europe, the patients kept on the table for hours, during which time, the utmost violence was committed, and the bladder was torn and shockingly mangled. This I never saw in Europe, but I have heard, since coming to Baltimore, that in the only two operations of Dr. Gibson, of which I can get a history, the one patient was kept 30 minutes on the table, and the other above forty five, and that they might die from absolute irritation a few hours afterwards, "I am ready to admit."

The last observation I shall make on the criticism, delivered by Dr. Gibson against the correctness of my practical deductions, is, that it is quite evident, he does not understand the facts adduced by me in support of them. He observes, "If Mr. Pattison had filled the bladder, and not poured the water into the pelvis, and then cut the prostate and neck of the bladder, and fascia, his class would have had a satisfactory demonstration of the fallacy of his principles, as the urine would have escaped, *pleno rivo*, and could not possibly have been detained by the fascia, or any similar structure." Unfortunately for Dr. Gibson's assertion, I did



fill the bladder with water, and not the pelvis, and the students had a most satisfactory demonstration, not of the fallacy, but the correctness of my principles; for, upon dividing the base of the prostate and neck of the bladder, the fascia necessarily being cut, the water, when forced from the bladder, flowed partly from the external wound, and was allowed partly to infiltrate into the cellular substance connecting the *bas fond* of the bladder to the rectum.

From my professional brethren I have nothing to fear. I am aware that there are some of them, who are not yet ready to assent to my doctrines, but I fondly, and confidently hope, that there is not one honest and honourable man amongst them, who will not confess, that I have inculcated my opinions in a modest, professional, and gentleman-like manner. Whether the principles I teach are correct or erroneous, time and experience alone can demonstrate. Yet this I can solemnly declare, that in adopting them myself, I did so from a conviction that they were just. That I may be wrong, is very possible, and so soon as I am convinced that I am so, I shall act, precisely as I did in adopting them; come before the public, and inform them, that I have been mistaken.

3dly. Dr. Gibson states, in his strictures, that I make false statements, and, to prove this, he uses with great freedom, the name of Dr. Physick. That gentleman's name is employed by him to confirm assertions, which I shall, from the most direct and positive evidence, prove to be false. Believing that the professor of anatomy was a man of honour, and one who would not allow himself to be made a *cat's-paw* by Dr. Gibson or any other professor, the moment I had glanced over the "*strictures*" I wrote him the following letter.



SIR,

I have just glanced over an essay of Dr. Gibson's, in which your name is employed to confirm statements, which are not consistent with truth. As I propose to morrow to write an answer to his Strictures, and as I should wish to know, before doing so, whether you have permitted him to use your name, I would beg answers to the following questions.

Did you not allow in my dissecting room, in the presence of Mr. Le Seaur, Dr. Eberle, and my brother, that the fascia, of which Le Seaur was taking a drawing, was a discovery? Were you aware of the existence of the prostate fascia previous to my coming to this country? Did you not request Dr. McClellan to carry me Colles's work? and, did you not, when I pointed out to you that the author had described the fascia, say, that his account was so confused, that it could not be understood? and further, that his having, or not having seen it, would militate nothing against the honour which was due to me, for the important practical conclusions, which I had deduced from that structure?

I have the honour to be, sir, your obedient servant,

GRANVILLE SHARP PATTISON.

*Friday Evening, September 15, 1820.*

*154 Walnut St.*

P. S. As I am anxious to return as soon as possible, to Baltimore, and wish to finish the MSS. to morrow, I shall expect an immediate answer.

Dr. Horner, delivered to me, the next day, the annexed card, with the contents of which I must confess, I was *really* astonished.

“Dr. Physick informs Mr. Granville Sharp Pattison, that he declines entering into any correspondence with him, respecting the controversy existing between him and Dr. Wm. Gibson.”

GRANVILLE SHARP PATTISON, Esq.

*Philadelphia, 16th Sept. 1820.*

As Dr. Horner begged me to write an answer to it, although I did not consider this necessary, I gave him for Dr. Physick the following.

Mr. Pattison is at a loss to understand, why Dr. Physick refuses to reply to the plain questions which were put to him in Mr. Pattison's letter of yesterday evening. Dr. Physick is, however, the best judge of what is most for his own interest.

*Saturday, 16th Sept. 1820.*

For DR. PHYSICK.

From the perusal of this correspondence, it will appear, that I have been unable to learn, whether Dr. Physick does, or does not warrant Dr. Gibson in employing his name. It is unfair in the extreme, for him to say, that he declines entering into “*any correspondence with me respecting the controversy existing between Dr. Wm. Gibson and myself.*”

His name having been used by my adversary, he has, to all intents and purposes, already entered into the controversy. From the conduct of Dr. Physick, we can therefore infer but one of two things. Either he must have allowed the professor of Surgery to make use of his name, for the purpose of giving currency to falsehoods, knowing them to be such,

and is still willing to let him take all the advantage arising from it;—that in fact, although his conscience would not permit him to assert a direct falsehood, yet it has allowed him to give an implied confirmation of one. Or, that his memory is so impaired, that it has not retained the recollection of an affair of very recent occurrence, and one, from the circumstances attending it, which was not very likely to have escaped, had it been in a sound state. I must leave Dr. P. to make his election in this, rather unpleasant dilemma.—It is painful to address a gentleman of Dr. P's. age and respectability with so much plainness, but his extraordinary conduct, on this occasion, has left me no choice.

The truth of my assertions, however, fortunately, can be confirmed, independently of Dr. Physick's authority. Dr. McClellan, a man whose truth is unquestioned, and who, were he to study self-interest, would support the opposite party, having been present at my interviews with Dr. Physick, when the subject of the prostate fascia was brought on the *tapis*, will confirm all I have advanced.

The first statement of mine, the truth of which is questioned, is, that Dr. Physick was not acquainted with the structure of the prostate fascia, before I came to this country. I would ask, if Dr. Gibson brings forward any evidence that he was? No, he adduces none! He merely asserts that it was well known to him. Now the circumstantial proof which I shall present, is, of itself, sufficient to prove the verity of my assertion. I am authorized by Drs. Cleaver, Jackson, Barnes, and Eberle, and I might name all the gentlemen educated in the Pennsylvanian school, to declare, that, although they had attended several of the courses of lectures delivered by Dr. Physick, and likewise the anatomical lec-

tures of Dr. Wistar, that this structure was not only not demonstrated to them, but that, previously to my arrival in the country, they had never heard of such a part.

2dly. The manner in which Dr. Physick demonstrated *last year, for the first time*, this structure, is sufficient to prove that, in spite of all my endeavours to prove to him that Colles had described the part, he still continued to consider me the discoverer. Dr. Gibson in mentioning this, states, "He merely showed the part, and read a passage or two from Mr. Pattison's paper.

My direct proof is equally satisfactory on this head. Dr. McClellan proves that it was with difficulty that I could persuade Dr. Physick, that Mr. Colles had really described the fascia, and Dr. Eberle says, that when Dr. Physick examined the part, in my class room, when it was fully dissected for the purpose of making the drawings, from which the engravings published in the Recorder were taken, that, even then, he was inclined to consider me the discoverer.

I stated in my "Reply" that, when I carried Colles to Dr. Physick and endeavoured to convince him that the anatomist had seen the prostate fascia he observed, "his description was so *confused*, that it was impossible to understand exactly what that author meant, and most unequivocally added, that his having or not having seen it, would, in no reason, take from the honour which was due to me for being the first who had the subject before the profession in an important and highly interesting point of view."

There are two assertions here; first, that Mr. Colles's description was said to be confused, and second that my credit remained unaffected, admitting that he had described the fascia. Dr. Gibson, still using Dr. Physick's name, contradicts



both of these assertions. That I was warranted in making this statement, will be most satisfactorily proved by the following extract from a letter which I received from Dr. McClellan:

“The question you asked me was, I believe whether I did hear Dr. Physick say any thing like what you represented, in your publication in the Recorder, respecting the clumsiness of Mr. Colles’s description, and its not taking away from your claim to the improvement &c. &c. To this I can most positively answer, I did. After you had read over to him again Colles’s description, he said, as nearly as I can recollect, “his description is so clumsy and confused that I don’t see how any body can make any thing out of it,” again he directly afterwards said “I don’t believe he had any clear idea of what he meant himself.” That Dr. Physick may have forgotten this conversation is possible, but it is quite certain I have not, and Dr. Gibson, by mistake perhaps, omitted to annex to his mention of this circumstance, the testimony I gave him on the subject. He asked me, when he met me, one morning shortly after your publication appeared, what I recollected on the subject, and I then told him just the same I have answered to your question.”

This extract not only confirms my statement, but proves that Dr. Gibson, in endeavouring to throw discredit upon it, did so in the face of direct evidence that I had written only that which was true.

From the consideration of the above facts it becomes evident that I am not only “*not guilty*” of the charges brought against me, but they most satisfactorily prove, that Dr. Gibson in making must have been aware that they were without foundation. In doing so, he was therefore guilty of the very accusation which he has adduced against me.



But the mistatements of the author of the "strictures," are not confined to these facts. If my answer be only reconsidered, it will be found, that in every charge made by him, he must have depended on the feeble hope of deceiving the public, knowing that the charges were not consistent with truth.

I will now prove that many of the most remarkable assertions made by Dr. Gibson are altogether unfounded.

It is asserted in the "strictures," that "the whole tenour of his, (Mr. Pattison's) conduct, from his arrival in the country to the present time has been empirical in the extreme, and such as would justify me, or any one else, in concluding he was at the bottom of the Norfolk advertisement, and *all the numerous puffs* which have appeared, at different times, in his favour."

There are a number of charges here, but they are all equally false, as I shall immediately show.

1st. My conduct has been empirical in the extreme since my settlement in America.

The first week I lectured in Philadelphia, there were published in Mr. Poulson's newspaper, some very flattering observations on my talents, as a teacher of Anatomy, As I felt unwilling to have my name brought forward, either favourably or unfavourably, in a newspaper, I sent a letter, of which the following is an extract, to the editor, in the hope that the request made in it to the gentlemen editing newspapers would have been attended to.

"The public, in general with justice, give the credit of such compositions either to the individual who is praised, or to some of his coadjutors. As I should feel lower in my

own estimation, could I suppose that this opinion was adopted in relation to me, I have to beg that you will assure the public, that the paragraph referred to, was introduced without my knowledge, and contrary to my wishes. I shall feel grateful if you, and the other gentlemen who edit newspapers, will, for the future, refuse insertion to any thing which may bring my name before the public."

---

Was this empirical in the extreme? Dr. Gibson would, I dare say, have said, had I not evidence to the contrary, that it was, and that my letter was a mere *blind* to deceive the public and induce them to believe that I had nothing to do with that, of which I was really the author. Unfortunately for this opinion the gentleman, who wrote the letter, (a person I have not the pleasure of being acquainted with,) mistaking the bearing of my letter, felt hurt, and sent another to the newspaper with his name, saying, that "his observations had been composed from friendly motives, &c. &c."

That he knew I was not the author of the Norfolk puff, I have already proved, and the numerous *puffs* of which he speaks, have no other existence than in his imagination.

I fear I have fatigued my reader, and shall therefore conclude this part of my subject by examining the following point. In my reply I stated, that he had, in his lectures, declared that no gorget was made sufficiently large to divide the whole body and base of the prostate gland. This the professor boldly denies, by stating, "*I never made an assertion of the kind.*" The extent of my observation was, that the prostate gland was not so easily divided, as some imagine, that in fact a gorget from three quarters of an inch to an inch might be employed, in most full grown subjects, without pro-

ducing such an effect. I did not suffer the observation to rest upon my assertion, for I performed the operation in presence of my class, with a gorget upwards of an inch, merely to put the matter to the *test*, and upon examination of the parts after the lecture, while several of the gentlemen were looking on, it was distinctly seen, that the gland *was not divided by the eighth of an inch.*"

That Dr. Gibson did make the assertion delivered by me, viz. that no gorget was made sufficiently large to cut the whole body and base of the prostate gland, I again affirm, and that I am warranted in doing so will appear, when I mention the fact, that, so struck was I with the absurdity of the assertion, that the moment it was delivered, I took a note of it, and handed it to Dr. Eberle, begging him to bear it in his recollection. This he has done, and I am authorized by him to state, that he recollects perfectly that it was made. As it was quite evident that the professor had got the whole of this lecture by rote, it is strange he should forget so important a part of it!!

The letter which follows, and which was sent me by its author merely from a sense of justice, without any application on my part, will prove that his memory is equally treacherous as it relates to his dissecting the parts immediately after the lecture, and his affirmation that the gland was not divided by the eighth of an inch.

---

Philadelphia, Alms-House Infirmary, Sept. 12, 1820.

DEAR SIR,

I observed this morning that Dr. Gibson, in a pamphlet, entitled "Strictures on Mr. Pattison's Reply to certain oral and written Criticisms," positively denies having

said in his lecture, that "no gorget was made sufficiently large to cut the whole body and base of the prostate gland," which so far as I know may be correct; but I can testify that Dr. G. did make use of the expression, immediately after the lecture, in a conversation with myself, which I will repeat as nearly as possible. After Dr. G's lecture, last winter, on lithotomy, I went down to the table where the subject was lying on which he had operated, (and the base of whose prostate he said he had not cut,) in order to satisfy myself whether the base of the gland was or was not divided. While I was endeavouring to satisfy myself on this point, the Doctor observed that he had that day used the largest gorget he could procure in the city, to show the absurdity of dividing the base of the prostate gland.

After a minute examination, however, I was inclined to believe that the base of the gland was divided, and accordingly made my opinion known to him, who thought it impossible. Upon repeating my conviction, he said, "I will risk the fate of my opinion that it is not divided." I then observed, if he had no particular use for the subject, I would be glad to dissect it, as I had heard both him and Mr. Pattison lecture on the subject, and was not yet satisfied. He answered that, "if the gentlemen had not *torn* it (the gland) in their examinations, he would dissect it himself, and show it to the class to-morrow." I replied, that "I believed no gentleman had handled it but myself, and was certain that I had done no violence to it." The next day, of course, I was very anxious to see the gland dissected loose from its connections: but he did not produce it. He acknowledged to the class, if my memory serves me, that the base of the gland was slightly divided, but that the fascia was untouched!! He



did not qualify his assertion by prescribing any dimensions to the gorget, but said "he never saw," or "no gorget was made sufficiently large to cut the whole body and the base of the prostate gland." (I am here only speaking of the conversation after lecture. The lecture itself I do not distinctly remember.)

The Doctor refers in his pamphlet, to *only one of two* operations he performed in this house, on dead bodies, in presence of several of the house surgeons and physicians, as well as a number of medical gentlemen of the city. Not being present myself, I can only say that my colleagues, who were present, inform me, that the beak of the gorget did not follow the groove of the staff in either instance. In the first, the gorget passed above the prostate gland, and was plunged into the body of the bladder. When he was informed of the route the gorget had taken, he replied "I thought so"—and in the other, to use his own language, "a small portion of the prostate, adjoining the urethra, where it opens into the bladder, was found undivided." So that he cannot from either of these cases, in my opinion, draw fair practical inferences.

As it has always been a maxim with me "to render unto Cæsar the things that are Cæsar's," you are at liberty to make what use of this communication the circumstances of your case may require.

I remain your friend,

G. GREEN.

To G. S. PATTISON, Esq.

*Professor of Surgery, Baltimore.*

I come now to consider, in the last place, whether the insinuation, thrown out by the author of the "*Strictures*," as to



my being a person of no eminence in my profession are, or are not just. I am aware that there is some appearance of indelicacy in taking up the defence of my character, on this head, myself, and were I not convinced that the interests of the university, with which I have the honour to be connected, demand from me a refutation of Dr. Gibson's insinuations, I should treat them with silent contempt. When I am told that the professor, in his valedictory lecture to the last course he delivered in the university of Maryland, declared that his attachment to the university of his native city was such, that he would rather hold a professorship in it, with a class of 20 students, than one in any other university in the world, I am at a loss to explain how this love should so soon have become converted into hate. This is strangely inconsistent; but perhaps the professor was so "*morally and physically exhausted*" at the time, as to be unable to prepare a valedictory, and said any thing that came first into his mind.

That I have some reputation in my profession, will be satisfactorily proved, by referring to the list of learned societies which have honoured me by electing me a member of their bodies. That those, to which I belong, are the most eminent and respectable in Europe, cannot be denied; and that their rolls contain the first medical names of the age, must likewise be admitted.—I would now ask what has he to show his claim to professional distinction?—what has he done? Has he ever written any thing? No! Has he ever discovered any thing? No! What then is he celebrated for.

"*It gives me pleasure to allow*" that he is an excellent modeller in wax.

Dr. Gibson seems desirous of making the impression, that I am a man of no experience. In his second letter, signed

Aristides, he observes, "It would appear too, from Mr. Pattison's own acknowledgment, that his *experience* in lithotomy had not been very considerable, since we hear of *two* operations *only* which he has performed, and from one of them the patient died, &c. &c." Again, in the present pamphlet, he remarks in italics, "Mr. Pattison's experience *is confined to two cases.*" In reply to these insinuations I have only to inform the Doctor, that, in writing the essay on lithotomy, it was not my object to publish all the operations I had performed, and as only two of the cases of stone, on which I had operated, illustrated my doctrines, these alone were mentioned; it was not, however, to be inferred from this, that these were the only cases on which I had operated. Dr. Gibson, as I learn from the gentlemen who attended his lectures, has *himself* performed the operation *fourteen times*; and when I learn from the same source, that he has operated *thirty times* for hernia!! and *sixty times* for cataract!! I have the satisfaction of thinking, that in going to Baltimore, I go to an excellent field for the practice of my profession.

As to the pitiful attempts made by Dr. Gibson, in his anonymous letter and strictures, to excite a prejudiced feeling against me from my being a foreigner, they are so characteristic of the puny soul that animates him, that they cannot fail to excite the disgust of the public, and my pity. I therefore shall conclude this statement by the publication of three letters, taken from sixteen, written in the same spirit, and furnished me by some of the first characters in Britain.

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#### LETTER OF SIR WILLIAM ADAMS.

26 Albermarle street, May 26, 1819.

MY DEAR SIR,

I cannot refuse myself the pleasure of addressing a

few lines to you, previously to your departure for America. For me to take pains to convince you that you have my most friendly wishes for your health and prosperity, I hope would be unnecessary.

It would afford me sincere pleasure were it in my power to contribute to either. In common with the rest of your friends, I deeply regret your departure, although selfish feelings ought not to be indulged, on such an occasion. For, when a professional man, of your zeal and knowledge, is placed in a situation to be pre-eminently useful in teaching his art to the professional men of a great and rising nation, we should look to the benefits which will, thereby, accrue to science and humanity, to the individual, through whose means such good is effected. I shall not hurt your modesty, by repeating in this letter, the degree of estimation in which you are held in this country. The letters, which you have obtained, from most of our eminent surgeons, must carry much greater weight with them than any thing I could say. Originating, as they do, from such high authority, I conceive if any further recommendation than your own well merited reputation were requisite to ensure the attainment of the honour you are solicitous to procure, these documents cannot fail to be successful.

That you may live long to render yourself, as heretofore, an example in your profession, an acquisition in every social circle, is the sincere wish of

My dear sir,

Yours, very faithfully,

W. ADAMS.

(Signed)

To Professor PATTISON.

LETTER FROM JOHN THOMPSON, M. D. F. R. S. E.

Professor to the Royal College of Surgeons, and Regius Professor of Military Surgery, in the University of Edinburgh.

*Edinburgh, 7th January, 1819*

MY DEAR SIR,

I received your letter this afternoon, and do not lose a moment in assuring you how happy I should be, to think I could in any way forward your views with regard to America.

To obtain the chair of Anatomy, now vacant in the first Medical University of the United States, is an object of ambition, worthy of a mind ardent as yours, in the pursuit and improvement of Anatomical science, and I cannot but heartily wish you success; however much I may regret our losing the benefit of your labours, in the country where your professional talents, knowledge and zeal are already so well known, and have been so eminently useful.

Your knowledge of anatomy, and the experience you have already acquired in teaching it, with your extensive and valuable collection of preparations, must give you a claim to the attention of the patrons of the University of Philadelphia, which no other candidate, I am convinced, can possibly possess.

With this impression of your merit I cannot but feel anxious that you should succeed in the attainment of your wishes, confident, as I am, that your appointment to the office you

are soliciting, must reflect as much honour, on those who confer it, as you can receive by obtaining it.

I remain, Dear Sir,

With the sincerest regard,

Yours, very truly,

(Signed)

JOHN THOMPSON, M. D.

To GRANVILLE S. PATTISON, Esq.

Lecturer on Anatomy, Glasgow.

# LETTER OF FRANCIS JEFFRAY, Esq.

EDITOR OF THE EDINBURGH REVIEW.

Dr. Chapman, &c. &c. }  
Philadelphia. }

Edinburgh, 28th December, 1818.

DEAR SIR,

I am afraid it may appear very presumptuous in me to present myself as a recommender of others, to a gentleman on whom I have no other claims, than what may be, rather ungratefully, founded, on the very polite attentions I have myself experienced at his hands.

As I am very much interested, however, in the success of the gentleman, on whose account I now address you, and very firmly persuaded, that it will, ultimately, be a source of satisfaction and comfort to yourself, I do not hesitate to risk the appearance of forwardness, that may attach to this application, and to bespeak your good offices in behalf of Mr. Granville Pattison, as a candidate for the vacant chair of Anatomy in your city of Philadelphia.



Of his professional qualifications, I do not pretend to speak, but from the report of others; but living, as I do, among the oracles of medical science, I may venture to say, that I do not remember an individual, in whose favour they have all testified so universally; and, from what I know of his history, I may add, that no person has, for many years, appeared in this country, who has united so devoted a zeal for his science, with so much acuteness and capacity, and so happy a faculty of communicating, and rendering interesting the knowledge he has attained.

His situation in Glasgow is extremely lucrative and respectable, and it may give you some idea of the attractions of his manner, and the extent of his reputation, when I mention, that, having been persuaded, last year, in addition to his proper scientific class, to deliver a course of more popular lectures on anatomy, he immediately assembled an audience of nearly three hundred persons, most of them unconnected with the profession of medicine.

He has spent a considerable time, both in London and Paris, with a more thorough and ardent dedication of himself to those pursuits, than I ever heard of in any other individual. In short, I verily believe, that a more accomplished anatomist, or a more successful lecturer, could not be found, and, for the honour of Scotland, as well as for the good of America, I cannot help being very much interested in his success.

Of course he will lay before you the most ample testimonials from the proper professional authorities; without these my recommendation would be ridiculous, and with them, I am very sensible it may well be regarded as idle and obtrusive. But I could not resist adding my insignificant tes-

timony in behalf of so much merit; and, endeavouring at least, to excite some attention to its claims, in the mind of an individual of great influence, whose former kindness to myself, encourages me to think, that he will at least pardon the liberty I have now taken.

I have the honour to be,

Dear Sir,

With great respect and esteem,

Your faithful and obedient servant,

(Signed)

F. JEFFRAY.



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January 11, 1881  
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